

conceding this view with regard to all the other causes of movement, Mr. Darwin finds a difficulty in accepting it as regards movement caused by curvature from a touch, or what is ordinarily called sensitiveness. On this point he remarks that the movement of Revolving Nutation (Sachs's term for "the continuous self-bowing of a whole shoot successively to all points of the compass") differs from that due to touch, in so far that in some cases the two powers are acquired by the same tendril at different periods of growth; and the sensitive part of the tendril does not seem capable of nutation. A more important cause of hesitation is the extraordinary rapidity of the movement. Mr. Darwin has seen the extremity of a tendril of *Passiflora gracilis*, after being touched, distinctly bend in twenty-five seconds, and often in thirty seconds; and he doubts whether it is possible to believe in such rapidity of growth as would account for such movement. In reference to this we may simply remark that instances are on record of extraordinarily rapid growth—as in the case of the flower-stalk of *Vallisneria* to the extent of half an inch in an hour or more—even without any abnormal irritation.

The student will find in Mr. Darwin's work a *résumé* of everything known to the present date on this interesting and curious department of Vegetable Physiology.

OUR BOOK SHELF

Report of the Meteorological Reporter of the Government of Bengal for 1874; and Administration Report of the Meteorological Reporter to the Government of Bengal for 1874-75. By W. G. Willson, Officiating Meteorological Reporter. (Calcutta: Bengal Secretarial Press, 1875.)

THIS Report, drawn up by Mr. W. G. Willson, who has acted as officiating reporter during the absence of Mr. Blanford, keeps up the high character of the previous reports of this Office. It contains not merely the dry details which form an integrant part of such reports, but also an able discussion of them both in their practical and theoretical bearings. As regards new observations, the most important are those from Sibsagar, situated in the north-east of the Assam Valley. The large barometric oscillation from 10 A.M. to 4 P.M., which on the average of the twelve months of 1874 amounted to 0.133 inch, will indicate their high strictly meteorological value. The hourly observations of the different instruments made on four days of each month at a number of the stations are a valuable piece of work, and the discussion of the results three or four years hence will be looked forward to with much interest by meteorologists. An admirable feature of these reports are the averages, corrected up to date in all instances, which are given with the discussion of each meteorological element, thus affording the means of an immediate comparison of the monthly results with the best averages that can possibly be had. Of these averages and comparisons we would direct special attention to those of the rainfall, which is of so great importance in Indian meteorology. Rainfall averages for different periods were prepared during the past year for the information of the Government of India. These have since been further amplified and corrected, and the present report gives the average monthly and annual rainfall for 146 stations, and incomplete averages for some months for other twenty-three stations. An example of the practical application of the rainfall discussions of the Office is given in the report. A forecast of the rainfall was called for by the Government from the Office in the latter part of July 1874, when considerable

apprehensions were entertained regarding the prospects of the principal rice-crop of the year. A comparison of the peculiarities of the rainfall up to the end of July with those of past years, and the general similarity of the meteorological circumstances with those of 1872, induced Mr. Willson to submit the opinion that the rainfall in the latter months of the monsoon would turn out as favourable as in 1872, a forecast which fortunately was fully realised.

The inter-relations of the pressure, temperature, winds, and rainfall are particularly inquired into, and some of the results are of very high importance, of which those referring to the weather of January, April, May, and June may be specially referred to. From a careful examination of the observations, Mr. Willson infers that during the hot weather months there is an upper westerly current from the heated plains of Northern India blowing towards the cooler regions of Assam above the southerly winds of the delta; and that, if this be so, the vapour carried from the Bay of Bengal by the southerly winds in the hot weather months is mostly diffused upwards and thence transferred by the upper westerly current to Assam, where it descends, and, meeting the cold north-easterly surface winds, its vapour is precipitated in the copious rains which fall in Assam in this season. According to this view, the excessive rainfall of Assam in May 1874 is completely accounted for by the very unusual strength of the southerly sea winds over the delta in that month; by the abnormally low surface-pressure and high temperature over the plains of Northern India, and by the relatively high surface-pressure and low temperature over Assam. It will be seen that there is here indicated a further development of the important practical question of the prediction of the character of the coming season. As regards these and many other questions of meteorology, still more important observations and discussions may be looked for from the reports of the new Meteorological Department now being organised by Mr. Blanford for the whole of India, including British Burmah and the islands of the Bay.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Oceanic Circulation

I HAVE, at Prof. Thorpe's suggestion (vol. xii. p. 514), compared the results obtained by the use of Prof. Hubbard's Table of the expansion of sea-water with that obtained from Muncke's, and find them almost identical. Both give 2 feet 6 inches as the amount by which the expansion of Column B* (lat. 23° N.) exceeds that of the Equatorial Column. Muncke's Table gives 3 feet 6 inches as the amount by which Column A (lat. 38° N.) exceeds that of the equatorial, while, according to Prof. Hubbard's Table, it is 3 feet 4 inches, being a difference of only 2 inches.

Dr. Carpenter objects† to my result on the ground that I have omitted the consideration of the inferior salinity of the equatorial column. Had I taken this into account, he thinks I would have found that it makes a difference in the opposite direction of about 1 foot in 1,026, which would more than neutralise the whole $3\frac{1}{2}$ feet of slope derived from temperature. I do not know upon what grounds he believes that the difference in salinity is so great between the equatorial and Atlantic columns. Certainly the researches of the *Challenger* Expedition do not warrant any such conclusion. It is true that there is an excess of salinity in the surface-water of the North Atlantic, but it does not extend to any great depth. This superior salinity of the warm upper stratum of the North Atlantic, it may be observed, is an additional evidence that the water is of Gulf-stream origin.

* Columns A, B, and C refer to Dr. Carpenter's section as given in my paper on the "Crucial Test" argument, read before the British Association at Bristol, and published in full in the *Philosophical Magazine* for September. A refers to North Atlantic column in lat. 38°, B to column in lat. 23°, and C to equatorial column.

† See NATURE, vol. xii. p. 533.

Through the kindness of the Hydrographer of the Admiralty, I have been favoured with all the observations made in the *Challenger* of the specific gravities of the Atlantic at intermediate depths between surface and bottom. From these observations it will be seen that there is scarcely any sensible difference between the mean specific gravity of the equatorial and the two Atlantic columns.

The following Table shows the mean specific gravities of the three columns:—

ATLANTIC.			EQUATORIAL.		
Depth in Fathoms.	I.	II.	Depth in Fathoms.	III.	IV.
	Lat. 38° 3' N. Long. 39° 19' W. Specific gravity at 60°.	Lat. 26° 58' N. Long. 22° 57' W. Specific gravity at 60°.		Lat. 1° 22' N. Long. 26° 36' W. Specific gravity at 60°.	Lat. 3° 8' N. Long. 14° 49' W. Specific gravity at 60°.
Surface	1'02684	1'02685	Surface	1'02616	1'02591
100	—	1'02732	50	1'02630	1'02598
150	1'02677	1'02658	90	1'02627	—
250	1'02641	1'02642	100	—	1'02643
400	—	1'02609	200	1'02607	1'02620
500	1'02608	1'02600	300	1'02618	1'02610
1500	1'02607	1'02620	400	—	1'02629
			1500	1'02618	1'02613
	1'026211 = mean of Column A.	1'02623 = mean of Column B.	Mean specific gravity of columns.	1'026181	1'026223
			Mean of the two.	1'026202 = Mean of Equatorial Column C.	

The mean specific gravity of the equatorial column as proved by the two soundings III. and IV. in the Table is 1'026202; and 1'026211 of sounding I. of the Table may be regarded as the mean specific gravity of the North Atlantic Column A, for the observations were made at a place on the same latitude, and only about two degrees to the east of that column. Consequently the specific gravity of Column A exceeds that of the equatorial by only '000009, a quantity which does not amount to one inch in 1,500 fathoms! Sounding No. II. of the Table, made at a place a few degrees to the east of Column B of the section, gives 1'02623, which may be regarded as the mean specific gravity of that column, and the more so as another sounding made in this region gives identically the same mean value. The difference between the Equatorial Column and Atlantic Column B in lat. 23° N. therefore amounts to only '000028, or 3 inches in 1,500 fathoms. It must of course be observed that as the specific gravities in the table are not taken at equal intervals the mean of the figures does not represent the mean specific gravity of a column. The number of fathoms represented by each separate value must be taken into account in determining the mean value of a column.

My result is, therefore, not materially affected, even after I have thus taken into account difference of salinity, and computed the amount of expansion according to Prof. Hubbard's Table. The surface of the North Atlantic in lat. 38° to be in static equilibrium must be 3 feet 3 inches above that of the equator, and in lat 23°, 2 feet 3 inches above it.

It is perfectly true that according to the gravitation theory the ocean is never in a state of static equilibrium, but it must be observed that as the surface-flow according to this theory is from the equator polewards, it is the equatorial column that is kept constantly below the level necessary to static equilibrium; hence, were I to make allowance for want of static equilibrium, I should make the slope greater than 3 feet 6 inches. Dr. Carpenter's objection that the force of my argument rests on the assumption that the sea is in equilibrium is based on a misapprehension of the problem, for in reality, by not making allowance for want of equilibrium, I give his theory an advantage which it does not deserve. Were the surface-flow from the North Atlantic to the equator, there would then be some force in his objection, for by leaving out of account want of equilibrium I would be making the slope greater than it should be. Dr. Carpenter states that his objection met the approval of General Strachey

and Sir William Thomson at the British Association meeting. If it did, it shows that they must either have misapprehended my argument or his objection to it.

I have again to remind Dr. Carpenter that "viscosity" can have nothing to do with the question at issue. The water has to flow up the "gradient," and that by means of gravity. This is mechanically impossible, whether water be viscous or not.

It is needless to quote the opinions of Lenz, Arago, and Pouillet. They were not in possession of sufficient data to enable them to determine the question with certainty. The question, be it observed, is not "Can difference of temperature produce circulation?" Everyone will admit that were there no other agencies at work but equatorial heat and polar cold, a difference of temperature would soon arise which would induce and sustain a system of circulation; but this condition of things is prevented by the equatorial waters being swept away by the winds as rapidly as they are heated. I submit that I have proved that this is the case in reference to the Atlantic. If I am wrong, let it be shown where my error lies.

JAMES CROLL

Edinburgh, Nov. 10

Refraction of Light and Sound through the Atmosphere

THERE is in Upper Thibet a plateau called the "Kyan Chu Plain," on which phenomena of mirage are frequently seen. The plain is at a height varying from 15,000 to 16,000 feet. A cold wind comes down from the surrounding mountains, while an exceedingly hot sun heats the ground. While marching through this plain on Aug. 19 I saw the mirage in perfection. A mountain in front of us, at a distance of about five miles, appeared to be situated on the border of a lake of a deep and rich blue. A shepherd with a flock of sheep seemed to wade through the water, and the reflection of each sheep was most distinct and sharp. The effect was so complete that one of my companions proposed to leave the pool of water at the side of which we had encamped for breakfast, in order to go to the borders of the lake.

I measured the temperature of the air at various heights from the ground. The following readings were obtained:—

Height above ground.	Dry Bulb.		Wet Bulb.	
5 feet ...	49°	...	32°	...
4 inches ...	55°	...	38°	...
1 inch ...	56°	...	39°	...

The ground at that place was stony, and no accurate measurement of its temperature could be taken. A few miles further on, however, a sandy ground was found to have a temperature of 90°.

The difference between the temperature of the ground and that of the air was painfully striking to me, as, owing to blisters, I had to walk with bare feet. My feet felt burning hot, while the remainder of the body was unpleasantly cold. The mirage was seen in its greatest perfection at about 9 o'clock A.M.

Such a condition of the atmosphere must, according to Prof. Reynolds, prevent any sound from being heard at a great distance, owing to its refraction upwards. Such was really the case. A rifle fired by the above-mentioned companion at a short distance remained almost unheard.

With regard to the question whether our better hearing at night is due to the absence of disturbing noises, or to the cause suggested by Prof. Reynolds, I wish to remark that the Upper Himalayas are particularly free from any disturbing noises, yet the increase in our power of hearing at night is most marked.

Sunnyside, Upper Avenue
Road, N.W., Nov. 20

ARTHUR SCHUSTER

Evidences of Ancient Glacier Action in Central France

HAVING read with much interest Dr. Hooker's contribution to NATURE on "Evidences of Ancient Glaciers in Central France," I am tempted to send you a few remarks which may interest those who look out for glacial phenomena wherever they travel.

When travelling in Auvergne with Sir William Guise in 1866, we unfortunately missed the transported erratics in the Tranteine Valley, described by Dr. Hooker. We saw, however, examples of what we believed to be ice-borne erratics, on more than one occasion, and consulted M. Lecoq on the subject at his residence at Clermont Ferrand. He had observed travelled boulders in certain localities, but, as mentioned in the note-book of Sir William Guise, "attributed to transport by snow many of the effects generally assigned to glacial action."

I would also ask attention to a subject which appears to me of considerable interest with regard to the age of the most modern